

is also interesting. Thus there are 304 endemic species, 232 Mascarene species, *i.e.*, plants confined to Bourbon, Mauritius, Madagascar, and the Comoros; 66 African but not Asian, 86 Asian but not African; 145 common to Asia and Africa; and 225 common to the Old and New World. If we take the percentages we have the following results:—29 per cent. endemic, 22 per cent. Mascarene, 21 per cent. common to the Old and New World, 14 per cent. common to Asia and Africa, 8 per cent. Asian but not African, and 6 per cent. African but not Asian. From this it is evident that one-half of the wild plants of the flora are restricted to the Mascarene Archipelago.

The orders containing the greatest number of species are the following:—Orchidaceæ, 79; Gramineæ, 69; Cyperaceæ, 62; Rubiaceæ, 57; Euphorbiaceæ, 45; Compositæ, 43; Leguminosæ, 41; Myrtaceæ, 20. There also 168 species of Filices, but it is rather unfair to consider the Filices as an order equivalent say to the Euphorbiaceæ or Myrtaceæ in the above enumeration.

The descriptive part of the flora is elaborated in the same manner as the colonial floras already published, and is, as already mentioned, almost entirely the work of Mr. Baker, with the exception of the Orchids, Palms, and Pandani. Any one acquainted with Mr. Baker's work will know that any detailed notice of the descriptive part of the present volume is superfluous.

W. R. MCNAB

OUR BOOK SHELF

Die Geologie. Franz Ritter von Hauer. (Vienna: A. Holder, 1877.)

It is a good sign both of the progress of geological study in Austria and of the value of this manual by the director of the Austrian Geological Survey, that a second edition of the work has been called for within three years of the date of its publication. A sample of the revised issue which has been sent to us fully bears out the description on its title-page that it is enlarged and improved. The original work, besides its clearly-expressed introductory chapters on general dynamical and mineralogical geology, is especially a valuable repertory of information regarding the structure and palæontology of the Austro-Hungarian monarchy. In the new edition, Ritter von Hauer is evidently doing his best to keep his manual abreast of the time. The book is well-printed, but the author is still in the hands of a very poor wood-engraver. The new cuts are as rude and feeble as ever.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Fritz Müller on Flowers and Insects

THE enclosed letter from that excellent observer, Fritz Müller, contains some miscellaneous observations on certain plants and insects of South Brazil, which are so new and curious that they will probably interest your naturalist readers. With respect to his case of bees getting their abdomens dusted with pollen while gnawing the glands on the calyx of one of the Malpighiaceæ, and thus effecting the cross-fertilisation of the flowers, I will remark that this case is closely analogous to that of *Coronilla*

recorded by Mr. Farrer in your journal some years ago, in which parts of the flowers have been greatly modified, so that bees may act as fertilisers while sucking the secretion on the outside of the calyx. The case is interesting in another way. My son Francis has shown that the food-bodies of the Bull's-horn Acacia, which are consumed by the ants that protect the tree from its enemies (as described by Mr. Belt), consist of modified glands; and he suggests that aboriginally the ants licked a secretion from the glands, but that at a subsequent period the glands were rendered more nutritious and attractive by the retention of the secretion and other changes, and that they were then devoured by the ants. But my son could advance no case of glands being thus gnawed or devoured by insects, and here we have an example.

With respect to *Solanum palinacanthum*, which bears two kinds of flowers on the same plant, one with a long style and large stigma, the other with a short style and small stigma, I think more evidence is requisite before this species can be considered as truly heterostyled, for I find that the pollen-grains from the two forms do not differ in diameter. Theoretically it would be a great anomaly if flowers on the same plant were functionally heterostyled, for this structure is evidently adapted to insure the cross-fertilisation of distinct plants. Is it not more probable that the case is merely one of the same plant bearing male flowers through partial abortion, together with the original hermaphrodite flowers? Fritz Müller justly expresses surprise at Mr. Leggett's suspicion that the difference in length of the pistil in the flowers of *Pontederia cordata* of the United States is due to difference of age; but since the publication of my book Mr. Leggett has fully admitted, in the *Bulletin* of the Torrey Botanical Club, that this species is truly heterostyled and trimorphic. The last point on which I wish to remark is the difference between the males and females of certain butterflies in the neurulation of the wings, and in the presence of tufts of peculiarly-formed scales. An American naturalist has recently advanced this case as one that cannot possibly be accounted for by sexual selection. Consequently, Fritz Müller's observations which have been published in full in a recent number of *Kosmos*, are to me highly interesting, and in themselves highly remarkable.

CHARLES DARWIN

Down, Beckenham, Kent, November 21

YOU mention ("Different Forms of Flowers," page 331), the deficiency of glands on the calyx of the cleistogamic flowers of several Malpighiaceæ, suggesting, in accordance with Kerner's views, that this deficiency may be accounted for by the cleistogamic flowers not requiring any protection from crawling insects. Now I have some doubt whether the glands of the calyx of the Malpighiaceæ serve at all as a protection. At least, in the one species, the fertilisation of which I have very often witnessed, they do not. This species, *Bunchosia gaudichaudiana*, is regularly visited by several bees belonging to the genera *Tetrapedia* and *Epicharis*. These bees sit down on the flowers gnawing the glands on the outside of the calyx, and in doing so the under side of their body is dusted with pollen, by which, afterwards, other flowers are fertilised.

There are here some species of *Solanum* (for instance *S. palinacanthum*) bearing on the same plant long-styled and short-styled flowers. The short-styled have papillæ on the stigma and apparently normal ovules in the ovary, but notwithstanding they are male in function, for they are exclusively visited by pollen-gathering bees (*Melipona*, *Euglossa*, *Augochlora*, *Megacilissa*, *Eophila*, *n. g.*, and others), and these would probably never insert their proboscis between the stamens.

In a few months I hope to be able to send you seeds of our white-flowered violet with subterranean cleistogamic flowers. I was surprised at finding that on the Serra (about 1,100 metres above the sea) this violet produced abundant normal fruits as well as subterranean ones, while at the foot of the Serra, though

it had flowered profusely, I could not find a single normal fruit, and subterranean ones were extremely scarce.

According to Delpino the changing colours of certain flowers would serve to show to the visiting insects the proper moment for effecting the fertilisation of these flowers. "We have here a *Lantana* the flowers of which last three days, being yellow on the first, orange on the second, purple on the third day. This plant is visited by various butterflies. As far as I have seen the purple flowers are never touched. Some species inserted their proboscis both into yellow and into orange flowers (*Danaïs crippus*, *Pieris aripa*), others, as far as I have hitherto observed, exclusively into the yellow flowers of the first day (*Heliconius apseudes*, *Colenis julia*, *Eurema leuco*. This is, I think, a rather interesting case. If the flowers fell off at the end of the first day the inflorescence would be much less conspicuous; if they did not change their colour much time would be lost by the butterflies inserting their proboscis in already fertilised flowers.

In another *Lantana* the flowers have the colour of lilac, the entrance of the tube is yellow surrounded by a white circle; these yellow and white markings disappear on the second day.

Mr. Leggett's statements about *Pontederia cordata* appear to me rather strange, and I fear that there is some mistake. In all the five species of the family which I know the flowers are so short-lived, lasting only one day, that a change in the length of the style is not very probable. In the long-styled form of our high- and *Pontederia* the style has its full length long before the flowers open. In my garden this *Pontederia* is visited by some species of *Augochlora* collecting the pollen of the longest and mid-length stamens; they are too large to enter the tube of the corolla, and have too short a proboscis to reach the honey; they can only fertilise the long-styled and mid-styled forms, but not the short-styled.

Among the secondary sexual characters of insects the meaning of which is not understood, you mention ("Descent of Man," vol. i., p. 345) the different neurination in the wings of the two sexes of some butterflies. In all the cases which I know this difference in neurination is connected with, and probably caused by, the development in the males of spots of peculiarly-formed scales, pencils, or other contrivances which exhale odours, agreeable no doubt to their females. This is the case in the genera *Mechanitis*, *Dircenna*, in some species of *Thecla*, &c.

FRITZ MÜLLER

Blumenau, St. Catharina, Brazil, October 19

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS'S letter in *NATURE* (vol. xvii. p. 26) has directed attention prominently to the circumstance that two hypotheses have been submitted to the scientific world as explanations of the force and motions which Mr. Crookes had shown to exist—one by Prof. Osborne Reynolds, the other by myself.

Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented.

My explanation, on the other hand, is based on molecular motions which go on in the gas without causing any molar motion, and is independent of convection currents. Prof. Reynolds is therefore, I conceive, fully justified in denying that my theory has supplied any deficiency in his explanation. As he points out, the two explanations are incompatible; if either is correct, the other is wholly wrong.

It is easy to apply comparative tests to the rival hypotheses by

making a selection from Mr. Crookes's incomparable experiments, from the experiments by Mr. Moss and myself, and from instances of compressed Crookes's layers in the open atmosphere; but it is not easy to make the choice so as to bring the abundant evidence within the compass of a letter.

These tests might take various forms, of which perhaps the most direct is to ascertain whether the force is affected by variations in the convection current, as required by Prof. Reynolds's hypothesis, or is independent of convection, but increased when the heater and cooler are brought nearer together, as required by mine.

To test this Mr. Crookes mounted a radiometer in a receiver consisting of two unequal bulbs connected by a large tube. The movable portion could be transferred from one bulb to the other through the tube. In the small bulb the convection current is most impeded, and at the same time the heater and cooler are closest together. Mr. Crookes found that the motion of the radiometer was more rapid in the small bulb than in the large one, in conformity with my theory, and in opposition to Prof. Reynolds's. The same is the uniform drift of a vast number of other experiments by Mr. Crookes, and of those by Mr. Moss and myself, from which it appears that whenever the heater and cooler are made to approach there is an increase in the force, and that the force is not appreciably affected by variations of the convection current, or by its suppression.

This may also be proved, and quite conclusively, by observations not requiring apparatus. Drops in the spheroidal state and the drops which are often seen floating on the surface of volatile liquids, as, for example, the drops which run about on the surface of the sea in certain states of the weather when water drips from an oar, are supported by Crookes's layers of air intervening between them and the liquid beneath. Similarly a red-hot copper plate will float on water, supported on a Crookes's layer, and many other instances of a like kind might be adduced. In such cases, where the film of air is thin and for the most part horizontal, it is manifest that there is no opportunity for those convection currents to arise which are required by Prof. Reynolds's hypothesis, while in all of them there are the peculiar molecular motions of my theory.

The absence of convection currents which could produce an appreciable effect may also be proved in those radiometers of which the arms whisk round at a very rapid speed, and in many other cases that would take too much space to describe here.

Again, a tangential force which may be rendered considerable is an immediate consequence of my theory, but has no place as a consequence of Prof. Reynolds's. Now its presence has been verified by Mr. Moss and myself, and by Mr. Crookes in an exquisitely beautiful apparatus suggested for this purpose by Prof. Stokes, as well as, in a less degree, in all Mr. Crookes's apparatus with curved or crumpled discs.

Hence Prof. Osborne Reynolds's hypothesis is not the explanation of Crookes's stress. It alleges a cause which is in certain cases a *vera causa*, but not the cause of what is to be explained. So far as I can form a judgment, its merit was collateral, and not intrinsic. It was the first attempt at a reduction of the observed phenomena to known physical laws. Though not accounting for them, it was sufficiently plausible to attract the attention of Prof. Reynolds and other physicists. It thereby had the important effect of suggesting Dr. Schuster's most valuable experiment, which was the first that established the cardinal fact that the forces within a radiometer case are balanced.

The conclusion to which we are thus led by a purely experimental inquiry is supported by an examination of the chief theoretical assertions of Prof. Osborne Reynolds's letter, viz., 1. That an essential part of my explanation "is contrary to the law of the diffusion of heat in gases;" and 2. "That the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional."

Both of these statements have been set down by Prof. Osborne Reynolds in error; the first from not observing that the ordinary laws for the propagation of heat through a gas do not apply to compressed Crookes's layers; and the second from a misapprehension of the actual agency at work in radiometers and other similar apparatus. I will proceed to establish these two positions.

1. So long as a gas is in its ordinary state the distribution of the velocities of the molecules is the same in all directions, and when heat is imparted to the gas it does not disturb this uniformity of structure. The heat simply increases the mean velocity, and the actual velocities continue to be distributed about